

In connection with this class of observation it is interesting to remark that the mean error of the moon's tabular place deduced from the meridian observations of 1883 has been brought down to $+0.03s$. in right ascension and $+0.42$ in longitude. This result has arisen because in this year Prof. Newcomb's corrections to Hansen's tables have been applied in the *Nautical Almanac*, so that the comparison has reference to Hansen's theory without his empirical term of long period (intended to represent the direct action of Venus) and with an empirical alteration in the epoch of the inequality resulting from the indirect action of Venus. The mean error of Hansen's tables uncorrected was $+0.82s$. in R.A. for the year 1882.

The most important reference to the spectroscopic work is the following:—

"For the determination of motions of stars in the line of sight, 412 measures have been made of the displacement of the F line in the spectra of 48 stars, 91 measures of the δ lines in 19 stars, and two measures of the D lines in one star, besides measures of the displacements of the δ and F lines in the spectra of the east and west limbs of Jupiter, and in the spectra of Venus and Mars, and comparisons with lines in the moon or sky spectrum made in the course of every night's observations of star-motions, or on the following morning, as a check on the adjustment of the spectroscope. Some preliminary measures have also been made of the F line in the spectrum of the Orion Nebula. The progressive change in the motion of Sirius, from recession to approach, alluded to in the last two Reports, is fully confirmed by numerous observations since last autumn, and a change of the same character is indicated in the case of Procyon. A discussion of the measures of all the stars observed here, on which I am now engaged, shows that the results of the four periods—1875 June to 1877 May, 1877 June to 1880 December, 1881 January to 1882 March 10, 1882 March 11 to 1884 March 31, in each of which the instrumental conditions were different—accord generally within the limits of the probable errors, and that there is no systematic change from recession to approach, so that the presumption against error arising from defective instrumental adjustment appears to be strong."

Passing on to another branch of the work at present undertaken by the Observatory, that connected with photographs of the sun with the view to determine the amount of spotted area, &c., we learn that two important changes have been made. First, the heliograph, which up to the present time has only given us pictures 4 inches in diameter, has been altered, as was suggested two years ago by the Solar Physics Committee, so as to take pictures of 8 inches. This necessitated a new micrometer which has already been constructed. Again, the photographs taken in India under the auspices of the Solar Physics Committee are now sent to Greenwich to be reduced with those of the previous series, and the result is a considerable increase in the number of days for which photographs are available. Thus in the year 1883 the 215 days of Greenwich are supplemented by 125 days of India, making a total of 340 out of 365 days. In 1882 we had Greenwich, 201, India 142, making up 343.

There is nothing new to remark with regard to magnetical work. We may state however that the magnetic elements for the past year were determined to be as follows:—

Approximate mean westerly declination	} $18^{\circ} 25'$.
Mean horizontal force	{ 3.926 (in English units). 1.810 (in metric units).
Mean dip	{ $67^{\circ} 31' 10''$ (by 9-inch needles). $67^{\circ} 31' 36''$ (by 6-inch needles). $67^{\circ} 31' 59''$ (by 3-inch needles).

The doings of the Deal time-ball and Westminster clock are thus referred to:—

"As regards the Deal time-ball, after various delays the arrangement, referred to in the last Report, for sending a current to Deal and receiving a return-signal through the chronopher of the Post Office telegraphs, was brought into operation on February 29, and has worked well since. The change has necessitated some slight alteration in our arrangements in order that we may be able to receive the Westminster signal through the same wire which is now used for the Deal current and its return signal. There have been 16 cases of failure in the dropping of the Deal time-ball owing to interruption of the telegraphic connections, 12 under the old system, and 4 since the new arrangement with the Post Office. On 19 days the current was weak and required the assistance of the attendant to release the trigger, and on 9 days the violence of the wind made it imprudent to raise the ball.

"The errors of the Westminster clock have been under 1s. on 53 per cent. of the days of observation, between 1s. and 2s. on 30 per cent., between 2s. and 3s. on 13 per cent., between 3s. and 4s. on 3 per cent., and between 4s. and 5s. on 1 per cent."

THE NORTH CAPE WHALE

THE North Cape or Biscay whale belongs to the group of true *Balæna*, or smooth whales, *i.e.* those whales which have no fin on the back or furrows along the throat, as is the case with the so-called fin-whale group. It has most in common with the South Sea whale (*Balæna australis*). Its systematic name is *Balæna bis-cayensis* (Eschricht).

The habitat of the North Cape whale is limited to the north temperate zone of the Atlantic Ocean, whereas the Greenland whale is found most frequently in the closer vicinity of the Pole. Along the coasts of Europe the North Cape whale used to be found from the Mediterranean to the sea north of Norway, as far as the Beeren Island. Its true home, was, however, according to earlier writers who have dealt with the whale-fisheries in the preceding centuries, between Iceland and Norway, its original name—the North Cape whale—being derived from its frequent appearance around that promontory some centuries ago.

It visited the coasts of Central and South Europe regularly during the winter months, its favourite haunt appearing to be the Bay of Biscay. There it began to be pursued very early—perhaps as far back as the eleventh or twelfth century. In the fourteenth century the whale-fishery was an established industry here. It was also, according to the Icelandic Saga, "Kongespeilet," written in the twelfth century, already at that period largely caught by the Icelanders. It was called by the latter *släibug* (smooth-back), and it was in all probability the catching of the North Cape whale of which the bard Othar of Haalogaland, *i.e.* Nordland in Norway, gave such an interesting account before King Alfred the Great of England. He stated that its haunts were then the shores of Northern Norway.

The principal expeditions for catching the whale were, however, despatched from the Bay of Biscay, but as it became more and more scarce in this part, it was followed as far as Iceland, where the Biscay fishermen found formidable rivals in the old Icelanders. It was these expeditions to Iceland which brought the Greenland whale under the notice of the southerners, and from the beginning of the seventeenth century the Greenland whale fishery around Spitzbergen became the leading industry.

In the middle of the seventeenth century the Americans began to catch whales. The Biscay whale was then very plentiful around the east coast of North America, and from the ports of "New England" numerous expeditions

for hunting this species were yearly despatched. The Americans called it "black whale," a denomination which, by the bye, also applies to other kinds.

Its range on the shores of America seems to have fallen a little south of that of Europe. It is in fact most probable that the whale visited the coast of Florida during the winter months, perhaps even more southern latitudes. Northwards it might be found as far as the sea is free from ice, but several circumstances seem to indicate that it preferred a temperate zone, and that its appearance on the shores of Greenland were merely migratory visits during the hot season. It may in fact be assumed that the North Cape whale made its regular migrations like the Greenland whale; in support of which I may point out that from the thirteenth to the fifteenth centuries the whale-hunting in the Bay of Biscay was carried on only during the winter months, and around America was limited to the season between November and April, at all events on the coast of New England.

What is known as to the principal haunts of this species of whale is alone based on the reports we possess of its hunting in the preceding centuries.

From the eighteenth century we hear no more about the catching of the North Cape whale in European waters, and in the beginning of the present century it also ceased to be hunted on the shores of America in consequence of its great scarcity.

It is therefore exceedingly interesting to find that the North Cape whale is again appearing on the east coast of America in such numbers that its catching is being resumed.

On the coasts of Europe the whale has only been discovered twice during this century, viz. in 1854, when a young one was caught at Pampeluna, the mother escaping; and in 1877, when the carcass of one—thirty-six feet in length—was cast ashore in the Bay of Toronto in Southern Italy. The skeleton of the former was brought to Copenhagen by the late Prof. Eschricht, where it now is.

The discovery which I made in 1882 on the shores of Finmarken of remains of this species of whale, hunted there by the Dutch in the sixteenth century, gave rise to further investigations as to the probable reappearance also in these parts of the North Cape whale, and from reports and circumstances brought to my knowledge, I feel convinced that considerable numbers of the North Cape whale again yearly appear on the coast of Northern Norway, where they were once so common. I must indeed regret that to ascertain with positive certainty whether this is a scientific fact is very difficult for a scientist whose stay in a certain part for scientific research is limited to a month or so. I hope, however, to obtain substantial proof of my belief at no very distant date.

For a figure of the North Cape whale I may refer the reader to that published in May 1883 in the *Bulletin* of the American Museum of Natural History, New York.

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MEASURING EARTHQUAKES

I.—METHODS

IT is difficult to define the word earthquake in terms which will not cover cases to which the name is inappropriate. To say that an earthquake is a local disturbance of the earth's crust, propagated by the elasticity of the crust to neighbouring portions, is true, but the definition does not exclude, on the one hand, such tremors of the soil as are set up by the rumbling of a carriage, by the tread of a foot, or even by the chirp of a grasshopper, nor, on the other, those slow elastic yieldings which result from changes of atmospheric pressure, from the rise and fall of the tides, and perhaps from many other causes. One

writer, in his definition of the word, limits the name earthquake to disturbances whose causes are unknown—a course open to the obvious objection that if the study of earthquakes ever advanced so far as to make the causes perfectly intelligible we should, by definition, be left with no earthquakes to study. It must be admitted, however, that in the present state of seismology this objection has no force, for in assigning an origin to any disturbance likely to be called an earthquake, we have, so far, been able to do little more than guess at possibilities. The more practicable task of determining what, at any one point within the disturbed area, the motions of the ground during an earthquake exactly are has lately received much attention, and in this department of seismology distinct progress has been made.

Apart from its scientific interest, this absolute measurement of earthquake motion is not without its practical use. Though the recent sharp earthquake in the Eastern Counties has reminded us that no part of the earth's surface can be pronounced free from liability to occasional shocks, these occur so rarely in this country that English builders are little likely to let the risk of an earthquake affect their practice. If Glasgow or Manchester had

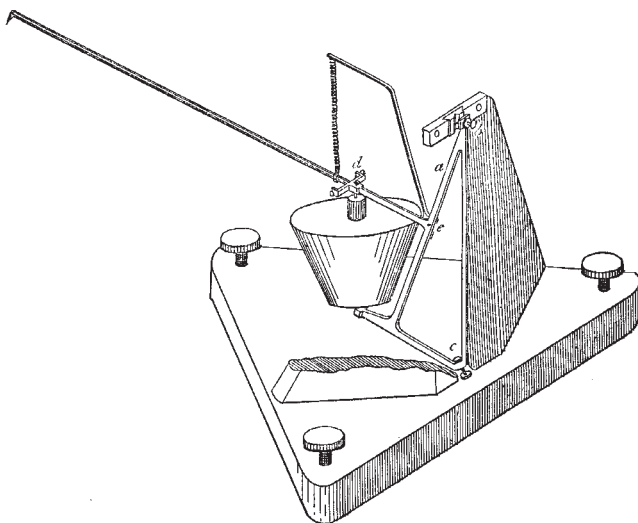


Fig. 1

been shaken instead of Colchester, the chimneys of the mills would, we suppose, have risen again in a few weeks no less tall than before. The case is different in an "earthquake country," such, for example, as some parts of Japan, where the present writer had the good fortune to experience, during five years, some three hundred earthquakes. Where the chances are that a structure will have to stand a shock, not once in a few centuries, but half-a-dozen times a month, the value of data which will enable an architect or engineer to calculate the frequency and amplitude of the vibrations, and the greatest probable rate of acceleration of the earth's surface, does not need to be pointed out.

To know how the earth's surface moves during the passage of a disturbance we must obtain, as a standard of reference, a "steady-point," or point which will remain (at least approximately) at rest. This is a matter of no small difficulty, for (as will be shown in a second paper) the motions during any single earthquake are not only very numerous but remarkably various in direction and extent. Most early seismometers were based on the idea that an earthquake consists mainly of a single great impulse, easily distinguishable from any minor vibrations which may precede or follow it. The writer's observa-